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The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participants Matter?

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This paper estimates the impact of participation, by gender, in the Grameen Bank and two other group-based micro credit programs in Bangladesh on labor supply, schooling, household expenditure, and assets. The empirical method uses a quasi-experimental survey design to correct for the bias from unobserved individual and village-level heterogeneity. We find that program credit has a larger effect on the behavior of poor households in Bangladesh when women are the program participants. For example, annual household consumption expenditure increases 18 taka for every 100 additional taka borrowed by women from these credit programs, compared with 11 taka for men.

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I. Introduction

This paper evaluates the effects of three group-based credit programs (Grameen Bank, Bangladesh Rural Advancement Committee [BRAC], and Bangladesh Rural Development Board's [BRDB] Rural Development RD-12 program) on a variety of household behaviors and on the intrahousehold distribution of resources. These programs are the major small-scale credit programs in Bangladesh that provide production credit and other services to the poor. In recent years, governmental and nongovernmental organizations in many low-income countries have introduced credit programs such as these targeted to the poor. Many of these programs specifically target women on the basis of the view that they are more likely to be credit-constrained than men, have restricted access to the wage labor market, and have an inequitable share of power in household decision making. Many of these programs earmark loans for production purposes only.[1] The Grameen Bank of Bangladesh is perhaps the best-known example of these small-scale production credit programs for the poor. The Grameen Bank, founded in 1976 by Muhammad Yunus, an economics professor, provides financing for nonagricultural self-employment activities. By the end of 1994, it had served over 2 million borrowers, of whom 94 percent were women. With loan recovery rates of over 90 percent, the Grameen Bank has been touted as among the most successful credit programs for the poor, and its model for group lending has been used for delivering credit in over 40 countries.

All three of the Bangladesh programs examined below work exclusively with the rural poor. Although the sequence of delivery and the provision of inputs vary some from program to program, all three programs essentially offer production credit to the landless rural poor (defined as those who own less than half an acre of land) using peer monitoring as a substitute for collateral. For example, the Grameen Bank provides credit to members who form self-selected groups of five. Loans are given to individual group members, but the whole group becomes ineligible for further loans if any member defaults. The groups meet weekly to make repayments on their loans as well as mandatory contributions to savings and insurance funds. Programs such as Grameen Bank, BRAC, and BRDB also provide noncredit services in areas such as consciousness-raising, training for skill development, literacy, bank rules, investment strategies, health,

[1] Some nonproduction lending does take place. In the Grameen Bank, e.g., a group fund, financed by the weekly contributions of group members, is used to make consumption loans to group members. More recently, Grameen has offered housing loans to group members as well.

schooling, civil responsibilities, and alteration of the attitude of and toward women.[2]

Very few studies have attempted to identify the causal effects of program participation. Previous studies that attempted to estimate program impact simply compared outcomes between participating and nonparticipating households. For example, a widely cited study similar in scope to ours (Bangladesh Institute of Development Studies 1990), carried out in the 1980s, did not address self-selection into the credit programs studied. To the extent that program participation is self-selective, it is not clear whether measured program effects reflect, in part, unobserved attributes of households (such as ability, health, and preferences) that affect both the probability they will participate in the programs (and the extent of that participation) and the household outcomes (schooling of children, labor supply, and asset accumulation) of interest. It is important not only to measure the impact of these credit programs on household welfare, but to determine whether targeting of credit toward women really matters. As Rashid and Townsend (1993) point out, the fungibility of credit within the household makes gender and other individual characteristics of borrowers potentially unimportant in loan usage and hence in the impact of loans on household outcomes such as those examined below. A finding that the gender of credit program participants matters in the determination of these outcomes is seemingly inconsistent with perfect fungibility.

This paper estimates the impact of participation, by gender, in each of the three group-based credit programs on women's and men's labor supply, boys' and girls' schooling, expenditure, and assets. We find that participation in these credit programs, as measured by quantity of cumulative borrowing, is a significant determinant of many of these outcomes. Furthermore, credit provided to women was more likely to influence these behaviors than credit provided to men. The method applied corrects for the potential bias arising from unobserved individual-, household-, and village-level heterogeneity. The study uses a quasi-experimental survey design to provide statistical identification of program effects in a limited information maximum likelihood framework. The survey design covers one group of households that has the choice to enter a credit program and may alter its behavior in response to the program, and a

[2] As part of Grameen Bank's social development program, all members are required to memorize, chant, and follow the "Sixteen Decisions." These decisions include "We shall keep our families small," "We shall not take any dowry in our sons' wedding, neither shall we give any dowry in our daughters' wedding," "We shall not practice child marriage," and "We shall educate our children." For details, see Khandker, Khalily, and Khan (1995).

''control'' group that is not given the choice of entering the program but whose behavior is still measured. Similarly, the identification of these programs' impact by the gender of the participant is accomplished on the basis of the comparison between groups of each gender with and without the choice to participate. Analyzing program impacts by comparing households in villages with programs and households in villages without programs suffers from the possibility that program placement is endogenous. These programs, whose professed goal is to better the lives of the poor, may have chosen villages in a conscious manner on the basis of their wealth, attitudes, or other attributes. We use a village-level fixed-effects method to circumvent the problem of village unobservables biasing our estimate of the impacts of these credit programs.

The remainder of the paper is organized as follows. Section II briefly discusses the role of the group in these programs and the peculiar advantages they provide women. Section III presents the empirical framework, and Section IV sets out the method of statistical identification using quasi-experimental aspects of the program and the special survey conducted in Bangladesh. Section V briefly describes the data. Section VI presents the results of estimating the reduced-form determinants of program credit and determinants of a set of household- and individual-level outcomes conditional on the quantity of program credit borrowed by gender. Section VII summarizes the results.

II. Group-Based Credit and the Gender of Participants

There are a number of reasons for group-based lending to be particularly attractive to women in rural Bangladesh and in other low-income societies. Very few women work in the wage labor market in rural Bangladesh. It is a conservative Islamic society that encourages the seclusion of women (*purdah*). Self-employment activities that produce goods at home for market sale are less frowned on culturally. Moreover, time in self-employment at home may jointly produce household goods such as child care. Although some of these production activities can be operated at low levels of capital intensity, for many a minimum level of capital is needed. This minimum is often the result of the indivisibility of capital items. For example, dairy farming requires no less than one cow and hand-powered looms have a minimum size. For other activities in which the indivisibility of physical capital is not an issue, such as paddy husking, transactions costs and the high costs of information place a floor on the minimal level of operations. In many societies these indivisibilities may not be consequential, but household income and asset wealth among the rural poor of many developing countries, including Bangladesh, are so low that the cost of initiating production at minimal economic levels is quite high. At very low levels of income and consumption, reducing current consumption to accumulate assets for this purpose may not be optimal because it may seriously threaten health (and production efficiency) and life expectancy, as shown in Gersovitz (1983). The production inefficiency associated with the lack of a women's labor market generates an incentive for borrowing capital to undertake women's self-employment that does not exist for men.

Why group-based credit? Group lending schemes may have an informational advantage over outside lenders: obtaining information about the actions of each member of a group by an outside lender would be costly and subject to misrepresentation. Group members can monitor each other with relative ease as well as train and assist low-productivity members. Social custom in rural Bangladesh restricts direct contact between potential female borrowers and (male) outside lenders. Even if the credit program organizer is a man, it is easier for a woman to interact with the organizer when in the company of a larger group of women. The informational advantages of group-based credit are thus likely to be greater for women than for men. This information advantage carries over to the issue of bundling credit and insurance. In the absence of insurance, adverse shocks may have an effect on the ability to repay loans as well as lower effort in the financed project and decrease income and consumption. Here again, the group is likely to have an informational advantage over outside lenders. Moreover, there is evidence that women are more prone to adverse shocks, related to pregnancy, illnesses associated with childbearing, and caregiving for other household members who fall ill, making them riskier clients for poorly informed outside lenders (Rashid and Townsend 1993). The credit programs evaluated in this paper bundle insurance with the provision of credit and rely on the information available to the group to administer this insurance.

The advantages of group-based credit for women described above are insufficient to generate an efficiency argument for targeting. In a model in which the household acts as though it is a single agent, husbands who are free to participate in the formal or informal credit market can borrow on behalf of their wives. In order for the incentives for borrowing capital to undertake women's self-employment to result in *women borrowers,* either both spouses must be credit-constrained or only the wife must be credit-constrained, and credit must not be fungible within the household. If multiperson households cannot be treated as single decision makers—if household allocations are the result of a process of interaction between members with different preferences (collective decision making)—then fungibility of funds within the household may not hold. Models of collective decision making as well as tests of their implications are now well represented in the literature, much of which is surveyed by Bergstrom (1995). There is a substantial literature in which the reduced-form demand for goods is related to some measure of the relative power or command over resources of one household member to another. Most of these empirical studies, many of which are surveyed in Strauss and Beegle (1994) and Bergstrom (1995), draw the inference that multiperson households cannot be treated as single decision makers. Consequently, credit might not be fungible within the household.

In this paper, we suggest and implement a method that treats survey data on participation in group-based credit programs as though this participation were generated by an experiment, with access to group-based credit ''randomly'' allocated to one sex or another, and that controls for self-selection into the program by these ''randomly'' chosen household members. However, any finding of different relative effects for participation in female and male credit programs should not be taken as a test of a collective model of household decision making. Peer monitoring in these group-based schemes is sufficiently close that households may have to carry out the funded project using the borrowed funds and the participants' time input as described in the application to borrow, even though both time and funds would be allocated differently in the absence of monitoring. If both a (landless) husband and wife are credit-constrained and only men have access to the wage labor market, then it may be optimal to borrow to fund a self-employment activity for the wife whether the decision is made by a single decision maker or collectively. The funding of self-employment activities will alter the shadow value of women's time, and perhaps the shadow value of children's time as well, and alter the allocation of goods through the familiar income and substitution effects. Similarly, group-based funding of a credit-constrained husband, with access to the labor market, whose ability to divert funds and effort is limited by the monitoring of the group, will likely have a very different impact on the shadow value of time and hence on substitution and income effects within the household in either a unitary or a collective model of decision making. The lack of fungibility of credit within the household may thus reflect close project monitoring and not necessarily a collective model of household decision making. Nonetheless, if self-selection and other sources of endogeneity are controlled for, any finding of differential effects of group-based credit on household outcomes by gender of program participant is not consistent with the fungibility of credit within the household.

III. Estimation Strategy

In this paper we estimate the *conditional* demands for a set of household behaviors, conditioned on the household's program participation as measured by the quantity of credit borrowed. The quantity of credit is, of course, only one measure of the flow of services associated with participation in any one of the group-based lending programs. As the Introduction has made clear, they are much more than just lending institutions. Nevertheless, the quantity of credit is the most obvious and well measured of the services provided.

Consider the reduced-form equation (1) for the level of participation in one of the credit programs (C_{ij}), where level of participation will be taken to be the value of program credit that household i in village j borrows:

$$C_{ij} = \mathbf{X}_{ij}\boldsymbol{\beta}_c + \mathbf{Z}_{ij}\boldsymbol{\pi} + \mu_j^c + \epsilon_{ij}^c, \tag{1}$$

where $\mathbf{X}_{ij}$ is a vector of household characteristics (e.g., age and education of the household head), $\mathbf{Z}_{ij}$ is a set of household or village characteristics distinct from the $\mathbf{X}$'s in that they affect C_{ij} but not other household behaviors conditional on C_{ij} (see below), $\boldsymbol{\beta}_c$ and $\boldsymbol{\pi}$ are unknown parameters, μ_j^c is an unmeasured determinant of C_{ij} that is fixed within a village, and ϵ_{ij}^c is a nonsystematic error that reflects unmeasured determinants that vary over households such that $E(\epsilon_{ij}^c|\mathbf{X}_{ij}, \mathbf{Z}_{ij}, \mu_j^c) = 0$.

The conditional demand for outcome y_{ij} (such as girls' schooling or women's labor supply) conditional on the level of program participation C_{ij} is

$$y_{ij} = \mathbf{X}_{ij}\boldsymbol{\beta}_y + C_{ij}\delta + \mu_j^y + \epsilon_{ij}^y, \tag{2}$$

where $\boldsymbol{\beta}_y$ and δ are unknown parameters, μ_j^y is an unmeasured determinant of y_{ij} that is fixed within a village, and ϵ_{ij}^y is a nonsystematic error reflecting, in part, unmeasured determinants of y_{ij} that vary over households such that $E(\epsilon_{ij}^y|\mathbf{X}_{ij}, \mu_j^y) = 0$. The estimation issue arises as a result of the possible correlation of μ_j^c with μ_j^y and of ϵ_{ij}^c with ϵ_{ij}^y. Econometric estimation that does not take these correlations into account may yield biased estimates of the parameters of equation (2) due to the endogeneity of participation in credit programs, C_{ij}.

The endogeneity of group-based credit may arise for the following reasons.

1. Placement of credit programs is nonrandom. It is unlikely that credit programs are allocated across the villages of Bangladesh in a random fashion. Indeed, program officials note that they often place programs in poorer and more flood prone areas, as well as in areas in which villagers have requested program services. Treating the timing and placement of programs as random can lead to serious mismeasurement of program effectiveness (Pitt, Rosenzweig, and Gibbons 1993). Consider the implications of a program allocation rule that was more likely to place credit programs in poorer villages than in richer ones. Comparison of the two sets of villages as in a treatment/control framework would lead to a downward bias in the estimated effect of the program on household income and wealth (and other outcomes associated with income and wealth) and could even erroneously suggest that credit programs reduce income and wealth if the positive effect of the credit program on the difference between ''treatment'' and ''control'' villages did not exceed the negative village effect that induced the nonrandom placement.

2. Unmeasured village attributes affect both the demand for program credit and household outcomes y_{ij}. Even if credit programs are randomly placed by the agencies involved, attributes of villages that are not well measured in the data may affect both the demand for program credit and the household outcomes of interest. These attributes include prices, infrastructure, village attitudes, and the nature of the environment, including climate and propensity to natural disaster. For example, the proximity of villages to urban areas may influence the demand for credit to undertake small-scale activities but may also affect household behavior by altering attitudes.

3. Unmeasured household attributes affect both the demand for credit and household outcomes y_{ij}. These attributes include endowments of innate health, ability, and fecundity, as well as preference heterogeneity. Consider the possibility that households are heterogeneous in their preferences with respect to the relative treatment of males and females within the household. It seems possible that households that are more egalitarian in their treatment of the sexes are more likely to provide additional resources to females, such as providing additional schooling to girls, and also more likely to have female household members participate in credit programs than otherwise identical but less egalitarian households. Ignoring this heterogeneity would wrongly ascribe to the credit program that part of the more egalitarian intrahousehold distribution of resources due to the more ''egalitarian'' preferences of households that self-select themselves into the program.

The standard approach to the problem of estimating equations with endogenous regressors, such as equation (2), is to use instrumental variables. In the model set out above, the exogenous regressors $\mathbf{Z}_{ij}$ in equation (1) are the identifying instruments. Unfortunately, it is difficult to find any regressors $\mathbf{Z}_{ij}$ that can justifiably be used as identifying instrumental variables. An approach motivated by demand theory is to use the price of the endogenous variable conditioned on as an identifying instrument. The most obvious measure of the price of participation in a credit program is the interest rate charged, but this is ruled out here since it does not vary across the sample. Even if interest rates varied across the sample, it is likely that some of this variation may reflect unmeasured household attributes unknown to us but known to the lender and likely to be part of the ϵ^y_{ij} error term and hence be an invalid instrument.[3]

Village fixed-effects estimation, which treats the village-specific component (μ_j) of the error as a parameter to be estimated, eliminates the endogeneity caused by unmeasured village attributes including nonrandom program placement. However, fixed-effects estimation raises issues of consistency and computational difficulty.[4] Even with village fixed effects, the endogeneity problem still remains if there are common household-specific unobservables affecting demand for credit and household outcomes, that is, if ϵ^c_{ij} and ϵ^y_{ij} are correlated. Lacking identifying instruments $\mathbf{Z}_{ij}$, we constructed the sample survey so as to provide identification through a quasi-experimental design.

[3] Another measure of the price of participation in credit programs is some proxy for the information costs associated with learning about these credit programs. To some extent, this depends on the qualities of the organizers and staff of the credit programs. Our survey collected information on the educational background, experience, age, and gender of organizers and other staff of the credit programs. There was a substantial number of missing values in these data, and these measured attributes tended to vary little across the sample. In any case, the validity of these variables as instruments requires that the credit programs allocate program organizers randomly across villages, which is uncertain.

[4] Measured program credit is a limited dependent variable since not all eligible households participate in the credit programs. Some of the household outcomes of interest—such as schooling of children and women's labor supply—are also limited dependent variables. As is well known, fixed-effects estimation in this case generally yields inconsistent parameter estimates without large numbers of observations on each fixed-effects unit. An exception is the fixed-effects Tobit estimator of Honoré (1992). Heckman (1981) provides Monte Carlo evidence that with eight or more observations per fixed-effects unit, the inconsistency problem becomes relatively inconsequential. The average number of target households per village in this study is 20.2. There are 87 village units in the data, 72 with credit programs, and joint estimation of credit use by gender (see below) with each household outcome (such as schooling or labor supply) implies that nearly 200 fixed-effects parameters need to be jointly estimated.

IV. Identification from a Quasi-Experimental Survey Design

A. *Eligibility Criteria as a Quasi Experiment*

In the classic program evaluation problem with nonexperimental data, individuals can elect to receive a treatment offered in their village (or neighborhood). The difference between the outcome (y_{ij}) of individuals who chose to receive the treatment and the outcome of those who chose not to is generally not a valid estimate of the treatment's effect if individuals self-select themselves into the treatment group. In the absence of any $\mathbf{Z}_{ij}$ (or panel data on individuals before and after treatment availability), one method of identifying the effect of the treatment is based on (presumed) knowledge of the distribution of the errors. This is the standard sample selection framework of Heckman (1976) and Lee (1976). If the errors are assumed to be normally distributed, as is common, the treatment effect is implicitly identified from the deviations from normality within the sample of treatment participants. The nonlinearity of the presumed distribution is crucial. If both the treatment and the outcome are measured as binary indicators, identification of the treatment effect is generally not possible even with the specification of an error distribution.

Our sample of households includes households in villages that do not have access to a group-based credit program. If credit program placement across the villages of Bangladesh is attentive to the village effects μ_j, identifying program effects by comparing households in nonprogram villages with households in program villages without controlling for the selectivity of program placement will generally result in biased estimates of program effects. Using a village fixed-effects estimation technique may remove the source of correlation between program placement and the behavior of interest; however, without further exogenous variation in program availability, the credit effect is not identifiable from a sample of self-selecting households as it is captured within the village fixed effects. In addition, the effects of any observed village characteristics that are thought to influence y_{ij}, such as prices and community infrastructure, are not identifiable. The parameter of interest, δ, the effect of participation in a credit program on the outcome y_{ij}, can be identified if the sample also includes households in villages with treatment choice (*program villages*) that are excluded from making a treatment choice by random assignment or some exogenous rule. That exogenous rule in our data is the restriction that households owning more than one-half acre of land are precluded from joining any of the three credit programs.[5] Data on the behavior of households exogenously denied program choice in this way are sufficient to identify the credit program effect. A comparison of the outcome y_{ij} between households with program choice and households without program choice, conditioning on village fixed effects and observed household and individual attributes, is an estimate of the program's effect on that outcome.

To illustrate the identification strategy, consider a sample drawn from two villages—village 1 does not have the program and village 2 does—and two types of households, landed ($\mathbf{X}_{ij} = 1$) and landless ($\mathbf{X}_{ij} = 0$). Innocuously, we assume that landed status is the only observed household-specific determinant of some behavior y_{ij} in addition to any treatment effect from the program. The conditional demand equation is

$$y_{ij} = C_{ij}\delta + \mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y + \epsilon_{ij}^y. \tag{3}$$

The exogeneity of landownership is the assumption that $E(\mathbf{X}_{ij}, \epsilon_{ij}^y) = 0$, that is, that landownership is uncorrelated with the unobserved household-specific effect. The expected value of y_{ij} for each household type in each village is

$$E(y_{ij}|j = 1, \mathbf{X}_{ij} = 0) = \mu_1^y, \tag{4a}$$

$$E(y_{ij}|j = 1, \mathbf{X}_{ij} = 1) = \boldsymbol{\beta}_y + \mu_1^y, \tag{4b}$$

$$E(y_{ij}|j = 2, \mathbf{X}_{ij} = 1) = \boldsymbol{\beta}_y + \mu_2^y, \tag{4c}$$

and

$$E(y_{ij}|j = 2, \mathbf{X}_{ij} = 0) = p\delta + \mu_2^y, \tag{4d}$$

where p is the proportion of landless households in village 2 that choose to participate in the program. It is clear that all the parameters, including the effect of the credit program δ, are identified from this design.

To illustrate the log likelihood maximized, consider the case of a binary treatment ($I_c = 1$ if treatment is chosen, 0 otherwise) and a binary outcome ($I_y = 1$ if the outcome is true, 0 otherwise). This is the most difficult model to identify in that nonlinearity arising from the choice of an error distribution is insufficient to identify the credit effect parameter δ. In the estimation results reported below, the treatment is actually measured as cumulative borrowing of program credit. Distinguishing between households not having a choice because they reside in a nonprogram village and households resid-

[5] The reasonableness of the exogeneity of landownership is discussed at length below.

ing in a program village that do not have a choice because of the application of an exogenous rule (landowning status), and suppressing the household and village subscripts i and j, we can write the likelihood as

$$\begin{aligned}
\log L(\boldsymbol{\beta}, \delta, \mu, \rho) \\
&= \sum_{\text{choice}} \log \Phi_2[(\mu_p^c + \mathbf{X}\boldsymbol{\beta}_c)\, d_c,\ (\mu_p^y + \mathbf{X}\boldsymbol{\beta}_y + \delta I_c)\, d_y,\ \rho d_c d_y] \\
&\quad + \sum_{\substack{\text{no choice} \\ \text{program village}}} \log \Phi[(\mu_p^y + \mathbf{X}\boldsymbol{\beta}_y)\, d_y] \\
&\quad + \sum_{\text{nonprogram village}} \log \Phi[(\mu_n^y + \mathbf{X}\boldsymbol{\beta}_y)\, d_y],
\end{aligned} \tag{5}$$

where Φ_2 is the bivariate standard normal distribution, Φ is the univariate standard normal distribution, μ_p^c are the village-specific effects influencing participation in the credit program in program villages, μ_p^y are the village-specific effects influencing the binary outcome I_y in program villages, μ_n^y are the corresponding village-specific effects in nonprogram villages, and $d_c = 2I_c - 1$ and $d_y = 2I_y - 1$.[6] The errors ϵ_{ij}^c and ϵ_{ij}^y are normalized to have unit variance and correlation coefficient ρ. Village-specific effects (μ_n^c) influencing the demand for program credit are not identifiable for villages that do not have programs.

The first part of the likelihood is the joint probability of program participation and the binary outcome I_y conditional on participation for those households that are *both* eligible to join the program (*choice*) and reside in a village with the program (*program village*). This part of the likelihood corresponds to the expectation (4d). Without regressors ($\mathbf{Z}$) that influence the probability of program participation but not the outcome I_y conditional on participation, the parameter δ, the effect of credit on the outcome y, is not sepa-

[6] Implicit in this setup is the assumption that the effect of the treatment (δ) is the same for all individuals, an assumption that is common in the program evaluation literature (Moffitt 1991). Furthermore, the model is not nonparametrically identified. That is, if the linear indices $\mathbf{X}_c\gamma$ and $\mathbf{X}_y\boldsymbol{\beta} + \delta I_c$ were replaced by nonparametric functions of the $\mathbf{X}$'s and I_c, the model is not identified. To ensure that the program effect estimated is not driven by the linear relationship between landholdings and the outcome variable, we have estimated the model while allowing for land to enter as a quadratic and successively higher-level polynomial. The program effect results reported below were not qualitatively altered by these changes.

rately identified from the parameters μ_p^y and $\boldsymbol{\beta}_y$ from this part of the likelihood. The second part of the likelihood is the (univariate) probability of a binary outcome I_y for landed households in program villages and corresponds to expectation (4c). These households are precluded from joining the program by their landed status. The last part of the likelihood is the probability of the outcome I_y for all households, landed and landless, in villages without a program and corresponds to expectations (4a) and (4b). If one of the regressors in $\mathbf{X}$ is a binary indicator of landed status, this part of the likelihood is required for identification. If the binary landed status variable is replaced by a continuous measure of landholding, as in the estimation reported below, then all the parameters of the model are identified without the last part of the likelihood. In this case, the parameter $\boldsymbol{\beta}_y$ in (3) is identified from variation in landholding within the program villages ($j = 2$), and a sample of nonprogram villages is not required.

Underlying identification in this model is the assumption that landownership is exogenous (as defined above) in this population. Although it is clearly nonstandard to use program eligibility criteria for purposes of identification in most instances of program evaluation, we think that its use is well justified here. Unlike the evaluation of job training programs, health/nutrition interventions, and many other types of programs, where lack of job skills, lack of health, and insufficiency in some other behavior are criteria for eligibility and the behaviors the programs directly act on, landownership is used as the primary eligibility criterion for these credit programs only to proxy for unverifiable and difficult to measure indicators of income, consumption, or total asset wealth. Landownership is simple to quantify, well known within the community, and unlikely to change in the medium term. Market turnover of land is well known to be low in South Asia. The absence of an active land market is the rationale given for the treatment of landownership as an exogenous regressor in almost all the empirical work on household behavior in South Asia. For example, in a classic paper in the field, Rosenzweig (1980) tested the implications of neoclassical theory for the labor market and other behaviors of farm households in India by splitting the sample on the basis of landownership, treating the sample separation criterion as nonselective. A number of theories have been set forth to explain the infrequency of land sales. Binswanger and Rosenzweig (1986) analyze the set of material and behavioral factors that are important determinants of production relations in land-scarce settings and conclude that land sales would be few and limited mainly to distress sales, particularly where national credit markets are underdeveloped. Rosenzweig and Wolpin (1985) set out an overlapping generations model incorporating returns to specific experience that has low land turnover as an implication and, using data from the Additional Rural Incomes Survey of the National Council of Applied Economic Research of India, find a very low incidence of land sales.

Even if landownership is exogenous for the purposes of this analysis, it is necessary that the ''landless'' and the ''landed'' can be pooled in the estimation. In order to enhance the validity of this assumption, we restrict the set of nontarget households used in the estimation to those with fewer than five acres of owned land. In addition, we include the quantity of land owned as one of the regressors in the vector $\mathbf{X}_{ij}$ and include a dummy variable indicating the target/nontarget status of the household. As the illustrative example of the identification strategy (eqq. [3] and [4]) makes clear, identifying the effect of target (landless)/nontarget (landed) status on behavior requires a sample of households from villages without a credit program.

B. Identification of the Impact of Gender-Specific Credit Using Single-Sex Groups

An important question of this research is whether various behaviors are affected differently by credit if the program participant is a woman or a man. For that reason, the reduced-form credit equation is disaggregated by gender:

$$C_{ijf} = \mathbf{X}_{ij}\boldsymbol{\beta}_{cf} + \mu^{c}_{jf} + \epsilon^{c}_{ijf} \tag{6}$$

and

$$C_{ijm} = \mathbf{X}_{ij}\boldsymbol{\beta}_{cm} + \mu^{c}_{jm} + \epsilon^{c}_{ijm}, \tag{7}$$

where the additional subscripts f and m refer to females and males, respectively. The conditional household outcome equation allows not only for separate female and male credit effects but also for different effects for each of the three credit programs:

$$y_{ij} = \mathbf{X}_{ij}\boldsymbol{\beta}_{y} + \mu^{y}_{j} + \sum_{h} C_{ijf}D_{jfh}\delta_{fh} + \sum_{h} C_{ijm}D_{jmh}\delta_{mh} + \epsilon^{y}_{ij}, \tag{8}$$

where D_{jfh} and D_{jmh} are village-specific indicator variables such that D_{jfh} takes the value of one in village j if credit program h (h = BRAC, BRDB, and Grameen Bank) has a female group in village j. At most one of D_{jfh} and D_{jmh} is positive since at most only one credit program is available in any sampled village. All three programs nominally require that C_{ijf} and C_{ijm} are not both positive; that is, an adult female and adult male cannot both participate. This rule seems not to be strictly enforced, and there are a number of households in our sample in which both an adult male and an adult female belonged to a credit group. As a consequence, we allow for a positive probability of participation for both men and women whenever the household is program-eligible and women's and men's groups are in the village.

Introducing gender-specific credit is not a trivial generalization of the econometric model. First, there are likely to be common unobservables that influence the credit program behavior of both women and men in the household. This requires us to model and estimate the demand for credit program participation separately for each sex, allowing for correlation between the errors ϵ^c_{ijf} and ϵ^c_{ijm}. Second, additional identification restrictions are required when there are both male and female credit programs with possibly different effects on behavior. Identification of gender-specific credit is achieved by making use of another quasi-experimental attribute of these programs and the survey. All program groups are single-sex, and not all villages have both a male and a female group. The sample includes some households from villages with only female credit groups, so that males in landless households are denied the choice of joining a credit program, and some households from villages with only male credit groups, so that landless females are denied program choice. In particular, of the 87 villages in the sample, 15 had no credit program, 40 had credit groups for both females and males, 22 had female-only groups, and 10 had male-only groups. The necessary assumption is that the availability of a credit group by gender in a village is uncorrelated with the household errors ϵ^y_{ij}, conditional on $\mathbf{X}_{ij}$ and the gender-specific village fixed effects μ^c_{jf} and μ^c_{jm}. These fixed effects sweep all village-level heterogeneity associated with the placement of credit program groups by gender. As each village had only one type of credit program available, and it is assumed that the type of credit program (BRDB, BRAC, or Grameen) is uncorrelated with the household errors ϵ^y_{ij}, conditional on $\mathbf{X}_{ij}$ and the village fixed effects μ_j, there is no need to model which of the programs members of a household join.[7]

While the likelihood given by (5) illustrates the general principle and method used, the actual likelihoods maximized are substantially more complex. Our method is a substantial generalization of the limited information maximum likelihood (LIML) methods pre-

[7] There are a very small number of individuals who belonged to credit programs that met in other villages. For example, there are some women in the sample who belonged to Grameen Bank groups even though there was not a Grameen Bank group in their village. These participation decisions were treated as exogenous in the analysis.

sented in Smith and Blundell (1986) and Rivers and Vuong (1988) for limited dependent variables. The likelihoods may contain trivariate normal distribution functions because the two credit equations (6) and (7) are being estimated simultaneously with a limited dependent variable outcome equation. In addition, the sample design is choice-based (see Sec. V below). In particular, program participants are purposely oversampled. The use of choice-based sampling somewhat complicates the econometrics but allows researchers to get the most statistical efficiency per dollar spent on data collection (Lancaster and Imbens 1991). Not correcting for the choice-based nature of the sample would lead to biased parameter estimates. The weighted exogenous sampling maximum likelihood (WESML) methods of Manski and Lerman (1977) were grafted onto the LIML methods described above in the estimation of both parameters and the parameter covariance matrix. The WESML estimates are obtained by maximizing a weighted log likelihood function with weights for each choice equal to the ratio of the population proportion to the sample proportion for that choice. The information required to construct these weights was directly measured in each of the surveyed villages. Before we drew a sample of households, a census of every household in each of the 87 randomly drawn survey villages (see Sec. V below) classified households as program-eligible (*choice*) or program-ineligible (*no choice*) on the basis of landownership and further classified program-eligible households in villages with credit programs as program participators or not. Sampling proportions varied across villages depending on village size and the size of each choice stratum. Oversampling of program-eligible households is equivalent to oversampling on the basis of landownership, a (maintained) exogenous variable, and thus does not in itself raise an issue of consistency of parameter estimates since this stratification does not constitute choice-based sampling. Even in villages without programs, households that would be program-eligible were oversampled. It is the oversampling of households that chose to participate in a credit program that requires the WESML technique. All household observations, including those without program choice, were weighted in the maximum likelihood results reported below.

To remind the reader of these crucial aspects of the maximum likelihood approach taken in this paper, the method is referred to as WESML-LIML-FE, which stands for weighted exogenous sampling maximum likelihood–limited information maximum likelihood–fixed effects. The Appendix to this paper provides an explicit characterization of the likelihood actually maximized as well as the asymptotic covariance matrix.

V. Survey Design and Description of the Data

A multipurpose quasi-experimental household survey was conducted in 87 villages of 29 thanas (subdistricts) in rural Bangladesh during 1991–92. The sample consists of 29 thanas randomly drawn from 391 thanas in Bangladesh, of which 24 had one (or more) of the three credit programs under study in operation, and five thanas had none of them.

Three villages in each program thana were then randomly selected from a list of villages, supplied by the program's local office, in which the program had been in operation at least 3 years. Three villages in each nonprogram thana were randomly drawn from the village census of the government of Bangladesh. A household census was conducted in each village to classify households as target (i.e., those that qualify to join a program) or nontarget households, as well as to identify program participating and nonparticipating households among the target households. A stratified random sampling technique was used to oversample households participating in one of the credit programs and target nonparticipating households. Of the 1,798 households sampled, 1,538 were target households and 260 nontarget households. Among the target households, 905 households (59 percent) were credit program participants.

Appendix table A1 presents the weighted means and standard deviations of all the independent variables used in the regression. Because the samples drawn are not representative of the village population, the means of the variables are adjusted by appropriate weights on the basis of the actual and sample distribution of the households covered in the study villages. The exogenous variables include a set of variables indicating the existence of nonresident relations of various types who are landowners. These types of households are potential sources of transfers that may importantly substitute for credit. Appendix table A2 presents summary statistics of the household- and individual-level outcomes that are examined in this paper disaggregated by various groups: participating and nonparticipating households in program areas, target households in nonprogram areas, and aggregates for all households in all areas. The survey design and data are described in greater detail in Pitt and Khandker (1995).

VI. Results

A. Comparing Estimators

In this section we present and interpret the results of estimating conditional demand equations of the form given by equation (8) for a set of household behaviors. In addition to WESML-LIML-FE estimates using the quasi-experimental identification restrictions set out in Section IV above, we present alternative sets of estimates that do not fully treat credit program placement and participation as endogenous. These alternative estimates are presented to illustrate the importance of heterogeneity bias.

Three of the four alternative estimates ignore self-selection into credit programs; two of these three treat the choice-based sampling nature of the survey appropriately and use WESML methods, whereas the other does not. The latter is actually more consistent with the maintained hypothesis of the naive model that choice—participation in credit programs—is exogenous, and thus fully consistent estimates are obtained by ignoring varying sampling proportions.[8] One of the three WESML estimators that ignores self-selection, labeled WESML-FE, does treat the possibly nonrandom allocation of credit programs across villages by including village effects, but it is presented only if the null hypothesis of exogeneity (based on the WESML-LIML-FE estimates) cannot be rejected. Models without village fixed effects include a set of village characteristics, consisting of five measures of village infrastructure, six goods prices, and two wage rates as regressors (see App. table A1), as is common in this type of cross-sectional analysis.

The third alternative estimator, labeled WESML-LIML, treats credit program participation as endogenous but also treats program placement as random and thus does not include village fixed effects. If the latter assumption is true, the WESML-LIML estimates are consistent and efficient, and the WESML-LIML-FE estimates are consistent but inefficient. If program placement is nonrandom, the WESML-LIML estimates are inconsistent. Hausman-like tests of the consistency of the WESML-LIML models were attempted, but the covariance matrices of the differences in the parameter vectors were not positive definite in every case tried. The test statistic computed is

$$(\hat{\boldsymbol{\beta}}_{\mathrm{FE}} - \hat{\boldsymbol{\beta}})'(\hat{\boldsymbol{\Sigma}}_{\mathrm{FE}} - \hat{\boldsymbol{\Sigma}})^{-1}(\hat{\boldsymbol{\beta}}_{\mathrm{FE}} - \hat{\boldsymbol{\beta}}),$$

[8] Furthermore, neither naive model deals with the possible nonindependence of the errors arising from multiple seasonal observations on some household behaviors (consumption and labor supply) or observations on more than one member of a household for other behaviors (schooling). This is not atypical of much of the applied literature in this area. If the exogeneity assumption is valid, ignoring nonindependence provides consistent parameter estimates but inconsistent estimates of the parameter covariance matrix. In the case of WESML-LIML, WESML-LIML-FE, and WESML-FE estimation, the parameter covariance matrices are computed using an asymptotic bootstrap method, essentially a variant of White's (1980) heteroskedasticity-consistent covariance estimator, to correct for the effects of nonindependent errors. The formula for this covariance matrix is presented in eq. (A12) of the Appendix.

where $\boldsymbol{\beta}_{\mathrm{FE}}$ and $\boldsymbol{\beta}$ ($\boldsymbol{\Sigma}_{\mathrm{FE}}$ and $\boldsymbol{\Sigma}$) refer to the WESML-LIML-FE and WESML-LIML parameter vectors (covariance matrices), respectively. Typically, the problem is that one or more of the diagonal elements of the covariance matrix ($\boldsymbol{\Sigma}_{\mathrm{FE}} - \boldsymbol{\Sigma}$) are very close to zero and sometimes negative. This problem is not uncommon in estimation problems of this kind. As the source of potential bias in the WESML-LIML estimates is correlation between village fixed effects and the regressors, we check for the presence of such a correlation by regressing the estimated village fixed effects on the full set of regressors in each of the WESML-LIML-FE models. This approach resembles that of Chamberlain (1984) to the specification of panel data models in which the fixed effects are explicitly modeled as linear functions of the regressors, except that we directly estimate the incidental parameters and use the second-stage regression of the estimated fixed effects on the regressors to establish that the fixed effects and the regressors are correlated. The estimated village fixed effects associated with female participation in credit programs, the μ^c_{jf} from equation (6), and the estimated village fixed effects associated with male participation in credit programs, the μ^c_{jm} from equation (7), are significantly (at the .05 level) correlated with the regressors $\mathbf{X}_{ij}$.[9] These fixed-effects parameters are repeatedly estimated in each model of behavior (labor supply, assets, schooling, and expenditure) presented below since the determinants of participation in credit programs, as measured by borrowing, are estimated jointly with each behavior in the maximum likelihood procedure, and thus this correlation between μ^c_{ij} and the observed determinants of credit characterizes all the behavioral models. Regressions of the estimated fixed-effects parameters associated with each (noncredit) behavior, the μ^y_j, on the set of regressors affecting behavior, $\mathbf{X}_{ij}$ and the C_{ijf} and C_{ijm}, reveal that these estimated fixed effects are correlated with the regressors in three of six cases (household expenditure, women's nonland assets, and girls' schooling) at the .05 level.[10]

[9] The test statistics are $F(14, 1{,}242) = 1.82$ and $F(14, 967) = 11.21$ for female and male credit, respectively. The fixed effects are those from the model, presented in table 1 below, in which only the reduced-form determinants of credit by gender are estimated with WESML bivariate Tobit fixed effects. To control for the possibility that the residuals of the regressions of fixed effects on the regressor may not be independent within a village, the parameter covariance matrix used in computing the test statistics is a variant of White's (1980) heteroskedasticity-consistent covariance estimator adjusted for village-specific random effects (App. eq. [A12]).

[10] These second-stage regressions of estimated village fixed effects are available from the authors on request.

B. *Village Externalities and the Interpretation of Village Fixed Effects*

One important drawback of estimating program impacts from data on two cohorts—those from villages with and without programs available—in which assignment to cohorts is nonrandom, that is, program placement is deliberate rather than random, is the possible misinterpretation of the village fixed effects. The discussion so far has treated the village effects as time-invariant attributes. But it is possible that credit programs can alter village attitudes and other village characteristics, perhaps through *demonstration or spillover effects,* and thus the attitudes of those who do not participate in the credit programs as well as those who do. The full effect of the program on behavior must then include any such village ''externalities'' and not just the direct effect on credit participants.

As an example, consider the limiting case in which program placement is in fact random but program activities, particularly those aimed at altering attitudes, successfully alter the views of nonparticipants in credit programs on a behavior such as the value of contraception and limiting family size. In this case, unobserved village contraception propensities would be correlated with program placement, but the causation would not go from village unobserved effects to program placement, but from program placement to village unobserved effects. In this scenario, programs are not placed in villages because of their relative attitudes on contraception, but rather the placement of program affects the attitudes of credit program nonparticipants in villages. Unfortunately, the only way these external effects can be measured is to have data on villages before and after introduction of the program.

This measurement problem implies that the placement of a credit program may *cause* a village effect in addition to a preexisting (time-invariant) village effect μ_j. Equation (3) then becomes

$$y_{ij} = \mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y + C_{ij}\delta + \Omega_j + \epsilon_{ij}^y,$$

where Ω_j represents the external effects of a program in a village and has the value zero if no program is located in the village. It is important to note that, whether or not there are nonzero credit program externalities, Ω_j does not affect the consistency of any estimate of δ, only its interpretation. The program effect parameter δ estimated by WESML-LIML-FE captures all program effects only if $\Omega_j = 0$ in all villages; that is, none of the village-specific heterogeneity in behavior is caused by programs. If village externalities exist ($\Omega_j \neq 0$), the WESML-LIML-FE estimate of δ represents only the effect of credit on program participants above and beyond its effect on nonparticipants in the village. If program placement is random and $\Omega_j \neq 0$, WESML-LIML is a more efficient estimator than WESML-LIML-FE, and the estimated δ has the same interpretation as for WESML-LIML-FE. If program placement is nonrandom, WESML-LIML is inconsistent. It is generally not possible to estimate the village externality Ω_j from a single cross section of data.

C. Demand for Credit

The results of estimating the credit equations (6) and (7), which are estimated jointly with the conditional demand equation (8) in every case in which LIML is applied, are presented in table 1. Since there are no endogenous right-hand-side regressors in the credit equations, they can be estimated separately from the conditional demand equations using WESML bivariate Tobit with village fixed effects. Implicit in these estimates is a set of restrictions on the parameters $\boldsymbol{\beta}_{cf}$ and $\boldsymbol{\beta}_{cm}$ of equations (6) and (7). In particular, the determinants of women's (men's) credit participation (the $\boldsymbol{\beta}$'s) are presumed to not depend on whether men (women) also have a choice of joining the credit program.[11] This restriction was tested and could not be rejected at common levels of significance ($\chi^2(28) = 22.6$, $p = .25$). Note that this does not necessarily imply that the presence or absence of a credit program for the opposite sex does not matter, only that it does not affect the slope parameters ($\boldsymbol{\beta}$). The "demand" curve may be shifted up or down, but such shifts are not statistically identifiable in this model since they are fully captured by the village-specific intercepts μ^c_{ij}. The other restriction is that the slope parameters $\boldsymbol{\beta}$ are common for the three credit programs. Again, the credit equations may be shifted up or down, but such shifts are not statistically identifiable in this model since they are fully captured by the village-specific intercepts μ^c_{ij}.

The variables describing the availability of potential sources of intrafamily transfers were not significant determinants of credit demand for either gender. The age and sex of the household head are apparently important determinants of credit demand for both women and men, but have opposite signs as between the sexes. Having a male head reduces the expected level of credit received by an eligible (as opposed to participating) woman by 47 percent and increases the expected level of credit received by an eligible male

[11] The idea is that there may be two regimes, each with different parameter vectors for each sex: a regime in which a sex is the only one able to choose to participate in a credit program and a regime in which both sexes can participate.

TABLE 1

WESML Bivariate Tobit Fixed-Effects Estimates of the Demand for Credit by Gender
Dependent Variable: Log of Cumulative Credit (Taka) since 1986

	Women		Men	
Explanatory Variables	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
Parents of household head own land	−.010	−.098	.042	.250
Brothers of household head own land	.036	.458	.170	1.622
Sisters of household head own land	.051	.621	−.034	−.339
Parents of household head's spouse own land	.005	.049	−.185	−1.126
Brothers of household head's spouse own land	.002	.034	−.027	−.295
Sisters of household head's spouse own land	.100	1.196	−.004	−.045
Log household land	.026	.540	.207	3.154
Highest grade completed by household head	−.021	−.352	−.029	−.334
Sex of household head	−2.068	−3.532	1.399	1.551
Age of household head (years)	.015	2.089	−.024	−2.373
Highest grade completed by an adult female in household	−.074	−1.754	−.026	−.458
Highest grade completed by an adult male in household	.029	.534	.142	1.802
No adult male in household	−1.257	−1.923		
No adult female in household			−.850	−.961
No spouse present in household	−.831	−2.483	−1.351	−2.951
σ (women's credit)	2.083	33.211		
σ (men's credit)			2.312	26.878
ρ (followed by *t*-statistic)		−.075 (−1.313)		
Observations	1,105		895	

by 33 percent. Increases in the age of the head of the household by 10 years are associated with a 5 percent increase in expected credit for women but a 5 percent decrease in expected credit for men. No spouse present in the household reduces expected credit for women by 23 percent and for men by 24 percent. Program credit is increasing with area of land owned for men but is not different from zero for women. A test of the hypothesis that the slope parameters in women's and men's credit demand are equal is strongly rejected ($\chi^2(14) = 50.94$, $p = .00$), reflecting to a large extent the opposite and significant effects of the sex and age of the household head as well as the land effect.[12]

D. *Household Expenditure and Women's Assets*

Table 2 presents estimates of the impact of participation in credit programs on the natural logarithm of total weekly expenditure per capita using all three rounds of survey data. All three WESML-LIML-FE female credit parameters are positive and statistically significant determinants of total expenditure, with no t-statistic less than 3.8, and are jointly significant ($\chi^2(3) = 19.03$, $p = .00$). In contrast, none of the male credit parameters has a t-statistic over 2.0, and the hypothesis that all the male credit parameters are zero cannot be rejected at the .05 level of significance ($\chi^2(3) = 4.11$, $p = .25$). The estimated female credit effects are approximately double the male credit parameters for the same credit program.[13] There are not substantially different effects among the three credit programs. At the mean, an additional one taka of credit provided women adds 0.18 taka to total annual household expenditure, as compared with 0.11 taka if the same amount of additional credit is supplied to men. The discussion in Section II suggests that one reason for the difference in the point estimates is the greater production inefficiency associated with women's time as a result of an absent women's wage labor market that is averted by access to credit.

The WESML-LIML parameter estimates of the determinants of (log) total expenditure in table 2 demonstrate the importance of the village fixed effects in the estimation. Women's credit effects are underestimated by WESML-LIML, and all three male credit parameters are negative and two (BRAC and Grameen) are statistically significant. The ''naive'' estimates presented in columns 1 and 2 of the table enormously underestimate the positive effects of program credit on total household expenditure. The effects of women's credit from BRAC and the Grameen Bank are underestimated by a factor of 10.

[12] The variables no adult females in the household and no adult males in the household were included as regressors because the adult education variables highest grade completed by an adult female in the household and highest grade completed by an adult male in the household are undefined when there are no adults (defined as a household member 16 years of age or older) of that sex in the household. Whenever there was no adult member of one sex in the household, the relevant highest grade completed variable was coded zero. The no adult variable thus picks up the difference between having zero as the highest number of years of schooling of adults of a particular sex and not having any adult of that sex in the household.

[13] Although the magnitude of these differences is large, the female credit parameters are not significantly different from the male credit parameters ($\chi^2(3) = 3.39$).

TABLE 2

ALTERNATIVE ESTIMATES OF THE IMPACT OF CREDIT ON PER CAPITA EXPENDITURE AND WOMEN'S NONLAND ASSETS

	LOG OF WEEKLY TOTAL PER CAPITA EXPENDITURE				LOG OF WOMEN'S NONLAND ASSETS				
EXPLANATORY VARIABLES	Unweighted OLS	WESML-OLS	WESML-LIML	WESML-LIML-FE	Unweighted Tobit	WESML-Tobit	WESML-LIML	WESML-FE	WESML-LIML-FE
Amount borrowed by female	.007	.007	.0340	.0394	.277	.182	.0425	.1151	.0318
from BRAC	(3.048)	(2.847)	(2.291)	(4.237)	(4.359)	(2.834)	(2.302)	(2.003)	(.356)
Amount borrowed by male	.010	.010	−.0161	.0192	.141	.110	.2589	.0878	.1005
from BRAC	(2.906)	(2.835)	(−1.658)	(1.593)	(1.615)	(1.214)	(2.367)	(1.007)	(.468)
Amount borrowed by female	.002	.003	.0258	.0402	.078	−.096	.0473	.2172	.1257
from BRDB	(.573)	(.906)	(1.723)	(3.813)	(1.040)	(−.949)	(.300)	(2.408)	(1.043)
Amount borrowed by male	.007	.007	−.0155	.0233	.234	.138	3.8329	.0244	.0334
from BRDB	(3.118)	(2.253)	(−1.788)	(1.936)	(3.934)	(1.608)	(3.340)	(.426)	(.141)
Amount borrowed by female	.003	.004	.0371	.0432	.232	.195	1.3484	.1989	.1131
from Grameen Bank	(1.400)	(1.765)	(2.174)	(4.249)	(4.402)	(3.318)	(1.452)	(3.950)	(1.317)
Amount borrowed by male	.001	.001	−.0225	.0179	.125	.096	.3377	−.0603	−.0457
from Grameen Bank	(.252)	(.325)	(−2.291)	(1.431)	(1.676)	(1.029)	(2.386)	(−.878)	(−.200)
ρ (women)			−.3897	−.4809			−.0168		.1136
			(−2.056)	(−4.657)			(−.198)		(1.325)
ρ (men)			.2999	−.2060			−.7656		−.0148
			(2.998)	(−1.432)			(−36.311)		(−.053)
Observations	4,567	4,567	5,218	5,218	1,517	1,517	1,757	1,757	1,757

NOTE.—Figures in parentheses are asymptotic *t*-ratios.

The pattern of the estimated correlation coefficients (ρ) suggests something about the nature of selection into the programs. The WESML-LIML-FE ρ's are both negative, more so for women, suggesting that, conditional on their village of residence and observed characteristics, low-expenditure households are more likely to participate in a credit program. That is, poorer households are being successfully targeted. Without conditioning on village of residence, the WESML-LIML ρ's suggest that the men of richer (higher-expenditure) households are more likely to join but the women of poorer (lower-expenditure) households are more likely to join.

Table 2 also presents estimates of the determinants of the value of nonland asset holdings by women. The asset variables are sex-specific rather than individual-specific in that they are defined as the total value of assets held by all individuals of each sex in the household. Thus no household contributes more than one observation to each of the sex-specific asset equations estimated. In addition, the mandatory savings component of these credit programs is not included in the calculation of nonland assets. A test of exogeneity, a test that the two ρ's are jointly zero, in the determination of the nonland asset holdings of women could not be rejected ($\chi^2(2) = 1.76$), and thus exogeneity is imposed in the WESML-FE estimates. The WESML-FE estimates find that participation in credit programs by women increases the value of their nonland asset holdings, whereas male participation does not. For women at the mean, every increase of 100 taka of credit from BRAC, BRDB, and Grameen Bank increases the value of their nonland assets by 15, 29, and 27 taka, respectively. Women's nonland assets seem to be the behavior for which the difference between the unweighted and weighted naive estimates is the greatest among those studied; that is, the choice-based nature of the sample matters most.[14]

E. Labor Supply

Table 3 presents alternative estimates of the impact of program credit on market labor supply including self-employment (log hours in the past week) by gender using all three seasonal rounds of the survey. The naive estimates substantially overestimate the effect of credit provided women on their labor supply. The exogeneity hy-

[14] The quality of asset data is typically suspect in household surveys, even more so when there is an attempt to break assets down by sex of ownership. The relative variance of the asset data is very high (see table A2), with many households reporting zero for women's assets. The male asset data were even more troublesome. We were unable to get any of the log likelihoods for the determinants of male assets to converge.

TABLE 3

Alternative Estimates of the Impact of Credit on Log Labor Supply by Gender

Explanatory Variables	Women (Hours in Past Month) Unweighted Tobit	WESML-Tobit	WESML-LIML	WESML-FE	WESML-LIML-FE	Men (Hours in Past Month) Unweighted Tobit	WESML-Tobit	WESML-LIML	WESML-LIML-FE
Amount borrowed by female	.028	.054	.0096	.0721	−.0117	.010	.013	−.2008	−.1813
from BRAC	(1.163)	(2.106)	(.144)	(1.884)	(−.128)	(1.290)	(1.623)	(−8.350)	(−5.884)
Amount borrowed by male	−.072	−.042	−.0908	−.0126	−.0448	.007	.002	.0246	−.1369
from BRAC	(−2.049)	(−1.103)	(−1.090)	(−.231)	(−.520)	(.676)	(.169)	(1.036)	(−2.155)
Amount borrowed by female	.131	.178	.2087	.0766	−.0139	−.002	−.000	−.2051	−.2308
from BRDB	(4.969)	(5.043)	(3.185)	(1.803)	(−.139)	(−.169)	(−.020)	(−7.635)	(−7.066)
Amount borrowed by male	−.007	.043	.0281	.0268	−.0144	.006	.001	.0172	−.1440
from BRDB	(−.303)	(1.278)	(.398)	(.682)	(−.181)	(.813)	(.072)	(.777)	(−2.129)
Amount borrowed by female	.116	.134	.1449	.1037	.0152	.012	.013	−.2175	−.2189
from Grameen Bank	(6.275)	(6.236)	(2.042)	(3.016)	(.162)	(1.910)	(1.803)	(−8.232)	(−6.734)
Amount borrowed by male	.081	.084	.0357	−.0229	−.0570	−.014	−.027	.0126	−.1592
from Grameen Bank	(3.012)	(2.406)	(.440)	(−.506)	(−.677)	(−1.594)	(−2.488)	(.522)	(−2.524)
ρ (women)			−.0173		.1255			.7151	.6564
			(−.196)		(1.062)			(11.698)	(7.461)
ρ (men)			.0415		.0560			−.0481	.4929
			(.435)		(.592)			(−.794)	(2.512)
Observations	5,693	5,693	6,602	6,602	6,602	5,846	5,846	6,914	6,914

Note.—Figures in parentheses are asymptotic *t*-ratios.

pothesis cannot be rejected for women's labor supply ($\chi^2(2) = 1.53$), and so exogeneity is imposed in the WESML-FE estimates. These estimates demonstrate a statistically significant positive effect of women's participation in the Grameen Bank on women's labor supply and the marginal significance of the women's BRAC and BRDB parameters. As both labor supply and credit are in natural logarithms, the credit parameters are the elasticities of (latent) hours of market labor supply with respect to credit. These elasticities are not large. Although statistically significant as a set, the largest of these labor supply elasticities, with respect to credit from the Grameen Bank, is only 0.104. In light of the relatively large elasticities of per capita household expenditure with respect to women's credit of around 0.4, it would seem that group-based credit provided women benefits household consumption presumably by increasing the productivity of women's market time rather than by increasing the supply of that time.

The conclusion that it is not an increase in market labor supply that underlies the increase in household consumption is reinforced by the male labor supply results. Both male credit ($\chi^2(3) = 98.66$, $p = .00$) and female credit ($\chi^2(3) = 53.11$, $p = .00$) reduce the labor time of adult male household members. A 10 percent increase in male group–based credit is associated with about a 1.4 percent decline in labor supply and a 10 percent increase in female group–based credit is associated with about a 2.1 percent decline in labor supply. As it seems unlikely that they are substituting home time for market time, the only conclusion to be drawn is that these negative cross effects reflect income effects. If the market value of men's time is unchanged by women's borrowing, their labor supply should fall if male leisure is a normal good. This is consistent with a variety of scenarios. One of them is that men already have ready access to nonprogram credit markets, so that program credit provides men mostly with rents proportional to the difference between the program and next-best alternative rates of interest. When this result was presented to those who manage and work in these credit programs in Bangladesh, they stated that it is consistent with their personal observation that the provision of credit from their programs tended to reduce men's labor supply. These labor supply results suggest that one other reason the effect of program credit on total household expenditure on goods is higher for women than for men is the increased consumption of leisure associated with male borrowing.

F. Schooling of Children

Table 4 presents estimates of the effects of participation in credit programs on the school enrollment status of boy and girl children

TABLE 4

Alternative Estimates of the Impact of Credit on School Enrollment: Girls and Boys Aged 5–17 Years

	Girls' Current School Enrollment					Boys' Current School Enrollment				
Explanatory Variables	Unweighted Probit	WESML Probit	WESML-LIML	WESML-FE	WESML-LIML-FE	Unweighted Probit	WESML Probit	WESML-LIML	WESML-FE	WESML-LIML-FE
Amount borrowed by female	.020	.015	.0693	.0119	−.0203	.024	.027	.0991	−.0028	.0394
from BRAC	(1.167)	(.938)	(1.990)	(.682)	(−.552)	(1.515)	(1.745)	(3.196)	(−.173)	(.917)
Amount borrowed by male	.044	.049	.0612	.0242	.0495	−.005	.002	.0113	−.0076	−.0040
from BRAC	(1.986)	(2.192)	(1.891)	(.897)	(1.152)	(−.231)	(.085)	(.333)	(−.341)	(−.107)
Amount borrowed by female	.011	.002	.0591	.0233	−.0099	.042	.035	.0956	.0793	.1210
from BRDB	(.612)	(.082)	(1.616)	(.804)	(−.220)	(2.346)	(1.588)	(3.066)	(3.106)	(2.573)
Amount borrowed by male	−.005	−.005	.0341	.0069	.0321	.022	.028	.0370	.0293	.0361
from BRDB	(.331)	(−.236)	(1.036)	(.309)	(.665)	(1.466)	(1.447)	(1.181)	(1.475)	(.934)
Amount borrowed by female	.023	.019	.0853	.0469	.0128	.053	.062	.1307	.0611	.1025
from Grameen Bank	(1.785)	(1.412)	(2.289)	(2.919)	(.334)	(4.141)	(4.461)	(4.022)	(3.644)	(2.364)
Amount borrowed by male	.100	.029	.0697	.0304	.0582	.053	.074	.0561	.0720	.0736
from Grameen Bank	(.614)	(.532)	(2.103)	(1.376)	(1.298)	(2.707)	(3.089)	(1.607)	(2.743)	(1.688)
ρ (women)			−.2728		.1648			−.4665		−.2192
			(−1.370)		(1.029)			(−2.490)		(−1.054)
ρ (men)			−.1409		−.1360			−.0222		−.0284
			(−.922)		(−.720)			(−.144)		(−.177)
Observations	1,269	1,269	2,885	2,885	2,885	1,341	1,341	2,940	2,940	2,940

Note.—Figures in parentheses are asymptotic *t*-ratios.

aged 5–17 at the time of the survey. In both cases, the exogeneity hypothesis could not be rejected, and so we shall reference only the WESML-FE estimates. These estimates demonstrate a strong and statistically significant effect of female Grameen Bank credit on the schooling of girls ($t = 2.92$). A 1 percent increase in Grameen Bank credit provided women is predicted to increase the probability of girls' school enrollment by 1.86 percentage points, at the mean. No other credit parameters are statistically significant. The relatively smaller effect of women's credit on their daughters' schooling for the other credit programs may reflect the close substitution of women's and girls' time in both the production of household goods and the self-employment activity. If mothers are drawn into self-employment, daughters' time may be used to replace the time mothers withdraw from household production (such as child care and food preparation). Although the Grameen Bank emphasizes the schooling of daughters as part of its social development program, there is no way to ascribe the higher girls' schooling effect to this attribute of its program.

The WESML-FE estimates of the determinants of boys' schooling presented in table 4 demonstrate a significant positive effect of women's credit from both Grameen and BRDB on boys' current schooling. Both the women's and men's credit variables are statistically significant determinants of boys' schooling ($\chi^2(3) = 22.21$ and $\chi^2(3) = 9.49$, respectively). A 1 percent increase in Grameen Bank credit provided women and men increases the probability of boys' school enrollment by 2.4 and 2.8 percentage points, respectively. A 1 percent increase in credit to women from the BRDB has the largest impact on boys' school enrollment, 3.1 percentage points. Unlike girls, boys are likely to be poor substitutes for women's/girls' time, and they are less likely to be drawn into the self-employment activity or into the production of household goods as a result of credit provided adult women.

VII. Summary

Group-based lending programs for the poor have become a focus of attention in the development community over the last several years. To date, there has been no comprehensive investigation of their impact on household behavior that has been sufficiently attentive to issues of endogeneity and self-selection. In addition, there is little evidence on whether production credit provided women has an effect on household outcomes different from that of production credit provided men. Evidence of such a difference is consistent with imperfect household fungibility.

TABLE 5

WALD TEST (χ^2) STATISTICS

	JOINT SIGNIFICANCE OF*			
OUTCOME VARIABLE	Credit Variables $\chi^2(6)$	Female Credit Variables $\chi^2(3)$	Male Credit Variables $\chi^2(3)$	Equality of Gender-Specific Credit Variables $\chi^2(3)$
Girls' schooling†	10.04	9.28	2.66	.92
Boys' schooling†	26.92	22.21	9.49	2.68
Women's labor supply†	16.23	14.15	.85	8.67
Men's labor supply‡	98.66	53.11	7.65	2.26
Per capita total expenditure‡	22.69	19.03	4.11	3.39
Women's nonland assets†	15.95	8.39	5.12	14.27

* Critical values are $\chi^2(3)_{.10} = 6.25$, $\chi^2(3)_{.05} = 7.82$, $\chi^2(3)_{.01} = 11.34$, $\chi^2(6)_{.10} = 1.64$, $\chi^2(6)_{.05} = 12.59$, and $\chi^2(6)_{.01} = 16.81$.

† Based on WESML-FE estimates.

‡ Based on WESML-LIML-FE estimates.

Using data from a special survey carried out in 87 rural Bangladeshi villages during 1991–92, this paper estimates the impact of female and male participation in group-based credit programs on a set of behaviors while paying close attention to issues of endogeneity. It uses the quasi-experimental design of the survey and the credit programs to identify the effects of program credit by gender of participant in a limited information maximum likelihood framework. In order to demonstrate the importance of unobserved heterogeneity, the paper presents alternative estimates of the programs' impact on a variety of household and individual behaviors using simpler approaches that do not control for varying levels of endogeneity. A comparison of our econometric method with the simpler alternative approaches clearly indicates the importance of our attentiveness to endogeneity in evaluating these credit programs and the mistaken conclusions that could be drawn from the simple ''naive'' estimates.

The paper provides separate estimates of the influence of borrowing by both men and women for each of three credit programs (the Grameen Bank, the Bangladesh Rural Advancement Committee, and the Bangladesh Rural Development Board's RD-12 program) on household expenditure, nonland assets held by women, male and female labor supply, and boys' and girls' schooling. Table 5 summarizes a set of joint hypothesis tests. We find that credit is a significant determinant of many of these outcomes. Furthermore, credit provided to women was more likely to influence these behaviors than credit provided to men. Credit provided women significantly affects all six of the behaviors studied at the .05 level of significance. Credit provided men does so in only one of six cases. Annual household consumption expenditure, the most comprehensive measure available of program impact, increased 18 taka for every 100 additional taka borrowed by women from these credit programs, compared with 11 taka for men. This evidence suggests that credit is not perfectly fungible within the household. While the point estimates by gender often differ greatly, statistical tests presented in table 5 reject the equality of men's and women's credit effects in only two cases, women's labor supply and women's nonland assets.

Appendix

Derivation of the WESML-LIML-FE Likelihood and the Asymptotic Parameter Covariance Matrix

Consider the reduced-form demand for program credit equations given by (6) and (7) and reproduced below:

$$C_{ijf} = \mathbf{X}_{ij}\boldsymbol{\beta}_{cf} + \mu^{c}_{jf} + \epsilon^{c}_{ijf} \tag{A1}$$

and

$$C_{ijm} = \mathbf{X}_{ij}\boldsymbol{\beta}_{cm} + \mu^{c}_{jm} + \epsilon^{c}_{ijm}, \tag{A2}$$

where C_{ijf} and C_{ijm} are the (latent) credit demands of females and males, respectively, in household i of village j; $\mathbf{X}_{ij}$ are a set of exogenous regressors; $\boldsymbol{\beta}_{cf}$ and $\boldsymbol{\beta}_{cm}$ are vectors of parameters; μ^{c}_{jf} and μ^{c}_{jm} are village fixed effects; and ϵ^{c}_{ijf} and ϵ^{c}_{ijm} are nonsystematic errors that may have nonzero covariance σ_{cfm}. The C_{ij} are limited dependent variables. Observed credit demands are

$$C^{*}_{ijk} = \begin{cases} C_{ijk} & \text{if } C_{ijk} > 1{,}000,\ k = m, f \\ 0 & \text{otherwise,} \end{cases} \tag{A3}$$

where the censoring threshold of 1,000 taka is smaller than the minimum loan size in our data. The conditional (on program credit) household demand equation allows for separate female and male credit effects and different effects for each of the three credit programs:

$$y_{ij} = \mathbf{X}_{ij}\boldsymbol{\beta}_{y} + \mu^{y}_{j} + \sum_{h} C_{ijf}D_{jfh}\delta_{fh} + \sum_{h} C_{ijm}D_{jmh}\delta_{mh} + \epsilon^{y}_{ij}, \tag{A4}$$

where y_{ij} is a behavior of household i of village j; μ^{y}_{j} is a village fixed effect; $\boldsymbol{\beta}_{y}$ is a vector of parameters; δ_{fh} and δ_{mh} are parameters; ϵ^{y}_{ij} is an error term; and D_{jfh} and D_{jmh} are village-specific indicator variables such that D_{jfh} takes the value of one in village j if credit program h (h = BRAC, BRDB, and Grameen Bank) has a female group in village j. Even though all three programs nominally require that C_{ijf} and C_{ijm} are not both positive—that is, an adult female and an adult male cannot both participate—this rule seems not to be strictly enforced, and there are a number of households in our sample in which both an adult male and an adult female from the same household belonged to a credit group. As a consequence, we allow for a positive probability of participation for both men and women in every household whenever the household is program-eligible and women's and men's groups are in the village. The error vector $\{\epsilon^c_{ijf}, \epsilon^c_{ijm}, \epsilon^y_{ij}\}$ is assumed to be distributed as joint normal with zero means and covariance matrix

$$\boldsymbol{\Sigma} = \begin{pmatrix} \boldsymbol{\Sigma}_{cc} & \boldsymbol{\Sigma}_{cy} \\ \boldsymbol{\Sigma}'_{cy} & \sigma^2_y \end{pmatrix}, \tag{A5}$$

where $\boldsymbol{\Sigma}_{cc}$ is the covariance matrix of the ϵ^c_{ijf} and ϵ^c_{ijm},

$$\boldsymbol{\Sigma}_{cc} = \begin{pmatrix} \sigma^2_{cf} & \sigma_{cmf} \\ \sigma_{cmf} & \sigma^2_{cm} \end{pmatrix}, \tag{A6}$$

σ^2_y is the variance of ϵ^y_{ij}, and $\boldsymbol{\Sigma}_{cy}$ is a vector of covariances between the credit errors ϵ^c_{ijf} and ϵ^c_{ijm} and ϵ^y_{ij}. The covariance matrix associated with households in which only one sex or the other has the choice of participating in a credit program as a result of the exogenous (conditional on village fixed effects) absence of a credit group for that sex in the village of residence of the household is

$$\boldsymbol{\Sigma}_k = \begin{pmatrix} \boldsymbol{\Sigma}_{k,cc} & \boldsymbol{\Sigma}_{k,cy} \\ \boldsymbol{\Sigma}'_{k,cy} & \sigma^2_y \end{pmatrix}, \quad k = f,\ m, \tag{A7}$$

where

$$\boldsymbol{\Sigma}_{k,cc} = \sigma^2_{ck}, \quad k = f,\ m, \tag{A8}$$

and

$$\boldsymbol{\Sigma}_{k,cy} = \sigma_{cky}, \quad k = f,\ m. \tag{A9}$$

This dimension of $\boldsymbol{\Sigma}_k$ is 2×2, whereas the dimension of $\boldsymbol{\Sigma}$ is 3×3 because only one of the credit equations is stochastic when only one sex has a choice to participate. The quantity of credit received by the sex without choice is deterministically zero. The submatrices defined in (A7)–(A9) are useful in neatly writing the joint likelihood as the product of the conditional and marginal likelihoods, which is the approach followed below whenever at least one of the credit behaviors, C^*_{ijf} and C^*_{ijm}, which are estimated jointly with Y_{ij}, is positive.

Consider the case in which the data consist of a binary realization on Y_{ij} and define $d_{ij} = 1$ if the realization is true and $d_{ij} = -1$ if it is false. We adopt the usual normalization $\sigma^2_y = 1$. The log likelihood consists of the following parts.[15]

[15] The likelihood detailed below can be readily altered to handle the case of a strictly continuous or censored (Tobit-like) behavior Y_{ij}. The logarithm of per capita household expenditure is a strictly continuous outcome, labor supply and the value of nonland assets are censored, and boys' and girls' current school enrollments are discrete in our data.

1. *Households without choice.*—

$$L1 = \log \Phi[(\mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y)\, d_{ij}],$$

where Φ is the standard normal probability function.

2. *Households for which only women have a choice.*—If $C_{ijf}^* = 0$ (women choose not to participate),

$$L2 = \log \Phi_2\left[\left(-\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_{cf} + \mu_{jf}^c}{\sigma_{cf}}\right),\ (\mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y)\, d_{ij},\ -\rho_{fy}\, d_{ij}\right],$$

where Φ_2 is the bivariate standard normal probability function and ρ_{fy} is defined as

$$\rho_{fy} = \sqrt{\boldsymbol{\Sigma}_{f,cy}\boldsymbol{\Sigma}_{f,cc}^{-2}\boldsymbol{\Sigma}'_{f,cy}}.$$

If $C_{ijf}^* > 0$ (women choose to participate),

$$L3 = \log \Phi\left\{\left[\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y + C_{ijf}\delta_{fh} + \boldsymbol{\Sigma}_{f,cy}\sigma_y^{-2}(C_{ijf} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cf} - \mu_{jf}^c)}{\sqrt{1-\rho_{fy}^2}}\right] d_{ij}\right\}$$
$$+ \log \phi\left(\frac{C_{ijf} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cf} - \mu_{jf}^c}{\sigma_{cf}}\right) - \log(\sigma_{cf}),$$

where $\phi(\cdot)$ is the standard normal density function.

3. *Households for which only men have a choice.*—If $C_{ijm}^* = 0$ (men choose not to participate),

$$L4 = \log \Phi_2[-(\mathbf{X}_{ij}\boldsymbol{\beta}_{cm} + \mu_{jm}^c),\ (\mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y)\, d_{ij},\ -\rho_{my}\, d_{ij}],$$

where

$$\rho_{my} = \sqrt{\boldsymbol{\Sigma}_{m,cy}\boldsymbol{\Sigma}_{m,cc}^{-2}\boldsymbol{\Sigma}'_{m,cy}}.$$

If $C_{ijm}^* > 0$ (men choose to participate),

$$L5 = \log \Phi\left\{\left[\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y + C_{ijm}\delta_{mh} + \boldsymbol{\Sigma}_{m,cy}\sigma_y^{-2}(C_{ijm} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cm} - \mu_{jm}^c)}{\sqrt{1-\rho_{my}^2}}\right] d_{ij}\right\}$$
$$+ \log \phi\left(\frac{C_{ijm} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cm} - \mu_{jm}^c}{\sigma_{cm}}\right) - \log(\sigma_{cm}).$$

4. *Households in which both women and men can choose to participate.*—Neither participates ($C_{ijf}^* = 0$ and $C_{ijm}^* = 0$):

$$L6 = \log \Phi_3\left[\left(-\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_{cf} + \mu_{jf}^c}{\sigma_{cf}}\right), \left(-\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_{cm} + \mu_{jm}^c}{\sigma_{cm}}\right),\right.$$
$$\left.(\mathbf{X}_{ij}\boldsymbol{\beta}_y + \mu_j^y)\, d_{ij},\ \rho_{fm},\ -\rho_{fy} d_{ij},\ -\rho_{my} d_{ij}\right],$$

where Φ_3 is the trivariate standard normal probability function and $\rho_{fm} = \sigma_{cfm}/(\sigma_{cf}\sigma_{cm})$. Only the woman participates ($C_{ijf}^* > 0$ and $C_{ijm}^* = 0$):

$L7$

$$= \log \Phi_2\left[\left(-\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_{cm} + \{[\mu^c_{jm} + \sigma_{cm}\rho_{fm}(C_{ijf} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cf} - \mu^c_{jf})]/\sigma_{cf}\}}{\sigma_{cm}\sqrt{1-\rho^2_{fm}}}\right),\right.$$

$$\left.\left(\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_y + \{[\mu^y_j + C_{ijf}\delta_{fh} + \rho_{fy}(C_{ijf} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cf} - \mu^c_{jf})]/\sigma_{cf}\}}{\sqrt{1-\rho^2_{fy}}}\right)d_{ij}, \tilde{\rho}_{my}d_{ij}\right]$$

$$- \log \phi\left(\frac{C_{ijf} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cf} - \mu^c_{jf}}{\sigma_{cf}}\right) - \log(\sigma_{cf}),$$

where

$$\tilde{\rho}_{my} = \frac{\rho_{my} - \rho_{fm}\rho_{fy}}{\sqrt{1-\rho^2_{fm}}\sqrt{1-\rho^2_{fy}}}.$$

Only the man participates:

$L8$

$$= \log \Phi_2\left[\left(-\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_{cf} + \{[\mu^c_{jf} + \sigma_{cf}\rho_{fm}(C_{ijm} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cm} - \mu^c_{jm})]/\sigma_{cm}\}}{\sigma_{cf}\sqrt{1-\rho^2_{fm}}}\right),\right.$$

$$\left.\left(\frac{\mathbf{X}_{ij}\boldsymbol{\beta}_y + \{[\mu^y_j + C_{ijm}\delta_{mh} + \rho_{my}(C_{ijm} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cm} - \mu^c_{jm})]/\sigma_{cm}\}}{\sqrt{1-\rho^2_{my}}}\right)d_{ij}, \tilde{\rho}_{fy}d_{ij}\right]$$

$$- \log \phi\left(\frac{C_{ijm} - \mathbf{X}_{ij}\boldsymbol{\beta}_{cm} - \mu^c_{jm}}{\sigma_{cm}}\right) - \log(\sigma_{cm}),$$

where

$$\tilde{\rho}_{fy} = \frac{\rho_{fy} - \rho_{fm}\rho_{my}}{\sqrt{1-\rho^2_{fm}}\sqrt{1-\rho^2_{my}}}.$$

Every sampled household contributes to one of the eight mutually exclusive and exhaustive parts of the likelihood. The complete WESML log likelihood $L(\theta)$, where θ is the complete set of unknown parameters, is the weighted sum of the individual household log densities, where the weight, w_{ij}, for household i in village j is the ratio of the population proportion to the sample proportion for each of the eight groups in the household's village:

$$L(\theta) = \sum_i \sum_j w_{ij}L_{ij}(\theta), \tag{A10}$$

where $L_{ij}(\theta)$ is the log density of household i in village j and corresponds to one of the eight parts of the log likelihood described above.[16]

[16] The quasi-experimental identification strategy used here is an example of the *regression discontinuity design* method of program evaluation in that it takes advantage of a discontinuity in the program eligibility rule to identify the program treatment effect (Van der Klauuw 1997). Two-stage instrumental variable estimation of a model of this type can be accomplished by treating as identifying instruments village dummy variables and a dummy variable for program eligibility interacted with all

The parameter covariance matrix is computed as

$$\left[-\frac{\partial \ln L(\hat{\theta})}{\partial\hat{\theta}\partial\hat{\theta}'}\right]^{-1}\left\{\sum_{i,j}\left[\frac{\partial \ln L_{ij}(\hat{\theta})}{\partial\hat{\theta}}\right]\left[\frac{\partial \ln L_{ij}(\hat{\theta})}{\partial\hat{\theta}}\right]'\right\}\left[-\frac{\partial \ln L(\hat{\theta})}{\partial\hat{\theta}\partial\hat{\theta}'}\right]^{-1}. \tag{A11}$$

The formula above is slightly altered when data on behavior Y_{ij} are available from multiple individuals in the same household, as in the case of girls' and boys' schooling, or when data are available from all three rounds of the survey, as in the case of labor supply and household consumption. When a third subscript k is added to index an individual within a household or a round (time period) for a household, the possibility of nonindependent residuals for all values of k for household i in village j is addressed in the estimation of the parameter covariance matrix by using the *sum* of the scores $\partial L_{ijk}(\hat{\theta})/\partial\hat{\theta}$ over all values of k for each household in calculating the covariance matrix of the first derivative vector:

$$\left[-\frac{\partial \ln L(\hat{\theta})}{\partial\hat{\theta}\partial\hat{\theta}'}\right]^{-1}\left\{\sum_{i,j}\left[\sum_{k}\frac{\partial \ln L_{ijk}(\hat{\theta})}{\partial\hat{\theta}}\right]\right.$$
$$\left.\times\left[\sum_{k}\frac{\partial \ln L_{ijk}(\hat{\theta})}{\partial\hat{\theta}}\right]'\right\}\left[-\frac{\partial \ln L(\hat{\theta})}{\partial\hat{\theta}\partial\hat{\theta}'}\right]^{-1}. \tag{A12}$$

Table A1 presents the weighted means and standard deviations of all the independent variables used in the estimations. Table A2 presents summary statistics of the household- and individual-level outcomes that are examined in this paper disaggregated by various groups: participating and nonparticipating households in program areas, target households in nonprogram areas, and aggregates for all households in all areas.

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the exogenous variables. The idea is that these exogenous variables have an effect on credit demand that depends on eligibility and availability but the outcomes of interest are not discontinuously affected by the exogenous regressors *conditional* on credit program participation.

TABLE A1

Weighted Means and Standard Deviations of Independent Variables

Independent Variable	Mean	Standard Deviation
Age of all individuals	23	18
Schooling of individual aged 5 and above (years)	1.377	2.773
Parents of household head own land?	.256	.564
Brothers of household head own land?	.815	1.308
Sisters of household head own land?	.755	1.208
Parents of household head's spouse own land?	.529	.784
Brothers of household head's spouse own land?	.919	1.427
Sisters of household head's spouse own land?	.753	1.202
Household land (in decimals)	76.142	108.54
Highest grade completed by household head	2.486	3.501
Sex of household head (1 = male)	.948	.223
Age of household head (years)	40.821	12.795
Highest grade completed by any female household member	1.606	2.853
Highest grade completed by any male household member	3.082	3.081
Adult male not present in household?	.035	.185
Adult female not present in household?	.017	.129
Spouse not present in household?	.126	.332
Amount borrowed by female from BRAC (taka)*	350.345	1,573.65
Amount borrowed by male from BRAC (taka)*	171.993	1,565
Amount borrowed by female from BRDB (taka)*	114.348	747.301
Amount borrowed by male from BRDB (taka)*	203.25	1,572.66
Amount borrowed by female from Grameen Bank (taka)*	956.159	4,293.36
Amount borrowed by male from Grameen Bank (taka)*	374.383	2,922.79
Nontarget household	.295	.456
Has any primary school?	.686	.464
Has rural health center?	.3	.458
Has family planning center?	.097	.296
Is *dai*/midwife available?	.673	.469
Price of rice	11.15	.85
Price of wheat flour	9.59	1
Price of mustard oil	52.65	5.96
Price of hen egg	2.46	1.81
Price of milk	12.54	3.04
Price of potato	3.74	1.59
Average female wage	16.154	9.613
No female wage dummy	.193	.395
Average male wage	37.893	9.4
Distance to bank (km)	3.49	2.85

Note.—Sample size is 87 villages, 1,757 households, and 9,215 individuals.

* Endogenous variable. Amount borrowed is the cumulative amount of credit borrowed since December 1986 from any of these three credit programs adjusted to 1992 prices.

TABLE A2

WEIGHTED MEANS AND STANDARD DEVIATIONS OF DEPENDENT VARIABLES

Dependent Variable	Participants	Nonparticipants	Total	Nonprogram Areas	Aggregate
Value of program borrowing by females (taka)	5,498.854 (7,229.351) $N = 779$	. . . $N = 326$	2,604.454 (5,682.398) $N = 1,105$	. . .	2,604.454 (5,682.398) $N = 1,105$
Value of program borrowing by males (taka)	3,691.993 (7,081.581) $N = 631$	. . . $N = 263$	1,729.631 (5,184.668) $N = 894$	. . .	1,729.631 (5,184.668) $N = 895$
Current school enrollment of girls aged 5–17 years (yes = 1)	.535 (.499) $N = 802$	.528 (.500) $N = 434$	.531 (.499) $N = 1,236$	.552 (.498) $N = 225$	.534 (.499) $N = 1,461$
Current school enrollment of boys aged 5–17 years (yes = 1)	.566 (.496) $N = 856$	.555 (.498) $N = 468$	.558 (.497) $N = 1,324$	.550 (.497) $N = 265$	.559 (.497) $N = 1,589$
Women's labor supply (hours per month, aged 16–59 years)	40.328 (7.478) $N = 3,420$	37.680 (71.325) $N = 2,108$	38.905 (7.934) $N = 5,528$	43.934 (74.681) $N = 1,074$	39.540 (71.432) $N = 6,602$
Men's labor supply (hours per month, aged 16–59 years)	202.758 (10.527) $N = 3,534$	185.858 (104.723) $N = 2,254$	191.310 (103.678) $N = 5,788$	180.94 (98.805) $N = 1,126$	189.477 (102.902) $N = 6,914$
Per capita household total expenditure (taka per week)	77.014 (41.496) $N = 2,696$	85.886 (64.820) $N = 1,650$	82.959 (58.309) $N = 4,346$	89.661 (66.823) $N = 872$	84.072 (59.851) $N = 5,218$
Female nonland assets (taka)	7,399.231 (293.02) $N = 899$	4,716.416 (19,901.035) $N = 542$	5,608.033 (23,509.09) $N = 1,441$	1,801.839 (6,287.491) $N = 292$	4,970.67 (21,649.42) $N = 1,733$

NOTE.—Standard deviations are in parentheses.

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